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THE ANNUAL MEETING will be held at the LONDON DAY TRAINING COLLEGE, Southampton Row, London, W.C. 1, on *Monday, 6th January*, 1930, at 2.30 p.m., and *Tuesday, 7th January*, 1930, at 10.0 a.m. and 2.30 p.m.

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EUCLID (I, 4) AND TIME-SPACE THEORY.

BY E. T. DIXON, M.A.

MAY I be permitted to make some comments on the interesting paper by Dr. Robb in the July number of the *Gazette*? The point he raises is indeed interesting; though, without his having said so, I should have been surprised to find that it was not generally known; that the Time-Space Theory may, in a limiting instance, be incompatible with Euclid's (I, 4). My comments must in a sense be critical, though I have nothing to say against his mathematics, so far as it is "pure," or merely symbolic; for even though we might not, with Macaulay, say that it is well known to every schoolboy, or was even known to Macaulay himself, it would be obvious enough to every modern-side schoolboy who has got as far as "conic sections"; provided he was willing to take some of Dr. Robb's technical terms in a more or less Pickwickian sense.

Dr. Robb has, of course, a perfect right, for the purposes of his symbolic argument, to define his technical terms as he pleases. If a dentist chooses to call a certain kind of toothpick a "spade," he is at liberty to do so; but he ought not to look down upon a rustic who calls a spade a "spade"; he ought not to talk of that as a "failure," or even as a "partial failure," of the rustic's intellect. The comments I propose to make here are, however, only critical of Dr. Robb in the sense that he does not seem to draw any such distinction between real and verbal (or symbolic) propositions. It does not appear clearly either in this paper, or in his book on the *Theory of Time and Space* (the only one of his, I regret to say, I have access to), whether the "elements" he is talking about are objective, *i.e.* controlled by "laws of Nature"; or merely subjective, controlled by "laws of thought"; or purely arbitrary things he can himself do what he likes with, say by free choice of definitions. A proposition of the latter class may be perfectly correct, and consistent with other propositions of the same class, but may have no real import which I can give to it, either in subjective conception, or in objective Nature. And an attempt to force real import upon it might well lead to undesirable consequences.

In the third paragraph of his paper, Dr. Robb says:

"The method of superposition employed by Euclid will not stand close criticism; although it is, I fear, too often regarded as good enough for schoolboys. This makes it all the more important to call attention to a definite instance in which the theorem breaks down in Time-Space Theory, which, so far as I am aware, is not generally known."

I should like to paraphrase the last sentence by saying that it makes it all the more important to call attention to this definite instance in which Dr. Robb tries to force on his Time-Space Theory a real import which it will not bear. Instead of finding fault with Euclid, who, though no more explicit about it

than Dr. Robb, probably intended his "Elements" to have real, objective import throughout, it would have been better had Dr. Robb tried to follow up what Euclid's line of thought actually was; and to realize in his own mind, as nearly as possible, what was in Euclid's, when the latter talked about figures in "space." Had he done so he would have realized that Euclid's "space" and Dr. Robb's Time-Space, were two different things; and it was therefore not to be expected that Euclid's theorems in the former would hold good in all cases in the latter. The "failure," if any, was not Euclid's, but Dr. Robb's.

Unfortunately pure mathematicians as such do not trouble themselves much about the real import of their symbols, and those of them who are physicists also, trouble only about the objective import they give them in the world of natural science. Few have attempted to follow out Euclid's line of thought, or to analyse what the subjective conception of space means to the man in the street. So far as I know the only one who has made anything like a successful attempt to do so is Hermann von Helmholtz; and he succeeded probably just because, besides being a mathematician (even if only an amateur), and a physicist (who worked in his own laboratory), he was a psychologist; which in his days was by itself enough to put him outside the pale of orthodox Science. But anyway Helmholtz followed Euclid's line of thought about superposition, and he further followed another line of thought, which may possibly also have been Euclid's; for in neither case can the lines of thought be directly deduced from Euclid's words, they can only be inferred indirectly from his deeds. But after all, that is really the only way in which anybody ever can follow the line of thought of anyone else; nobody can transfer an idea from his own mind to that of another man, in the sort of sense in which he might transfuse some of his blood into his veins; he can at best only act in such a way as to suggest to the other man a train of thought, which that other man has to work out for himself. It is only because in the affairs of everyday life we have become so habituated to such suggestions, to such trains of thought started by actions, particularly by words of other people, that we think of our ideas as having been transferred, by means of language, to us from other people. In these everyday affairs trial and error have so moulded our thoughts and actions, that they have largely become intuitive and automatic; and for this reason whenever we come upon fundamental or Common Sense ideas psychological analysis becomes very difficult; and philosophers get out of it by talking about "Axioms." But this is just what Helmholtz did not do; he was, in effect, a pragmatist, and he talks about his premisses as hypotheses, though later writers deliberately misrepresent him in this respect.

The two lines of thought I wish to follow here, both of which possibly, and the former of which almost certainly, run parallel to the thoughts of Euclid, seem to be indicated very clearly in Helmholtz second and fourth hypotheses. In the former he writes:

"*Es wird die Existenz von beweglichen aber in sich festen Körpern, bezielich Punctsystemen, vorausgesetzt*, etc." The *Existenz*, or real import, attributed to his rigid bodies or point-systems, is of course the objective import of physical science; the same sort which Professors Einstein and Eddington attribute to their measuring-rods, which appears to be good enough for them; though I hope Dr. Robb does not include them in his category of "schoolboys." Dr. Robb perhaps would take himself out of this category on the ground that his own Time-Space Theory required no such hypothesis about rigid bodies. As I should put it, this contention would be that his Theory was a purely symbolic one; as such it may be, and no doubt is, perfectly correct. But I do not think Dr. Robb would wish to maintain such a position. He would not be satisfied to say that his Theory had no application to the world of physical science, even if, as a "behaviorist," he took no interest in subjective conceptions of space.

It is not, however, Helmholtz' hypothesis about rigid bodies, or Euclid's assumption of the method of superposition, tacitly made, which Dr. Robb is challenging directly in his paper, so much as Euclid's conception of an angle. And I am bound to say that, though Dr. Robb's symbolic analysis is quite all right, when he comes to give it real import, he here makes what Aristotle might have called a fallacy of the ambiguous middle. Referring to his diagram, he says that " the triangle " (*OQP*) " is not determined in all its dimensions when the magnitudes of the sides *OQ* and *QP* are given, along with the fact that they are normal to one another." This is supposed to indicate a " failure " of Euclid's I, 4 ; it must, therefore, be taken that Dr. Robb mentally substituted for his term " normal " an Euclidean term, such as " at right-angles," with the Euclidean implication that all right-angles are equal to one another. But the way Dr. Robb appears to define his term " normal " is not at all the same as Euclid's definition of a right-angle ; which, in effect, is by superposition. He does it practically in his Proposition I, 11 ; and it is noteworthy that he calls this proposition a " Problem " ; as also he calls his first proposition of all, to which I shall refer in a moment. Would Dr. Robb on that account ignore these theorems, as mere exercises in geometrical drawing ? I hope not ; to me they seem a most valuable indication that Euclid recognized the importance of *doing*, as opposed to mere *being* ; even more valuable perhaps than that afforded by his I, 4. But anyway, though Euclid unnecessarily lays it down as an " axiom," that all right-angles are equal, this problem, I, 11, might be used to prove that axiom (by means of his I, 4, that is, by superposition), as well as to define a " right-angle," " in a plane," and therefore also " in space." But it could not be used in Dr. Robb's diagram, albeit he says that *OQ* and *QP* are " normal " to one another. Dr. Robb's conclusion that in this case Euclid's I, 4 " failed " is thus a fallacy of ambiguity. His term " normal " is not the same as " at right-angles." The true conclusion is that Dr. Robb's diagram did not represent, even pseudo-metrically (that is, even if not supposed to be drawn to scale) a " plane " figure, or a figure in Euclidean space. It no doubt represented correctly enough a figure in Time-Space, in Dr. Robb's sense of the term, but in that case he ought not to talk of his Time-Space as a " space."

But now let us ask what there is about our space-conception which ought to prevent our confusing a Time-Space with a " space " as ordinarily thought about. The difference is just that property of our space-conception which, so far as I know, Helmholtz was the first to express in words ; or to try, so to express, since the words he used seem to have been generally misunderstood. Helmholtz called this property, which he assumes in his fourth hypothesis as belonging to our space, *Monodromie* ; a term which is perhaps unfortunate, inasmuch as it seems to mean that in space I need only turn myself round once, to be " as you were." It is not, however, intended to exclude some vague possibility that I might turn round more than once, describing perhaps some kind of spiral, before being " as you were " ; at least I do not think Helmholtz had any such idea. But it seems to me just possible that Helmholtz, though logically recognizing the need for such a premiss for the geometry of space, had actually no clear idea of any alternative. Anyway, what he really required was only a premiss to the effect that I can turn myself round to a position " as you were," *at all.* I doubt whether Helmholtz even realized that Euclid, with his marvellous intuition, actually has such a premiss. And, again, it is a premiss of which he makes use by doing something with it, in one of his " Problems," despised by idealistic mathematicians as mere doings, as they also despise his methods of superposition. His " postulates " are all treated by this type of mathematician with a contempt which they do not deserve ; and the third of them, " that a circle may be described from any centre, at any distance from that centre " (Simpson), is a practical equivalent for Helmholtz' *Monodromie* ; for Euclid defines a circle as a " plane figure contained by one line," which implies not only that the line is a boundary in the plane, but that

it is a closed one; *i.e.* that you can pass right round and get to your starting point without having to retrace your steps.

Well then, in Dr. Robb's Time-Space diagram you *could not* rotate QP round Q, to QR; at most, in a "limiting case" you could only rotate it as far as his "optic line" through Q, after an "infinite" amount of rotation. Dr. Robb's Time-Space diagram has not what Helmholtz called *Monodromie*. You cannot, by metrical transformation, pass a "separation" line into an "inertia line," to use Dr. Robb's nomenclature. Of course you might do it symbolically, by an "imaginary" transformation; but we are here trying to give real import, time and space; and the real experience of mankind amply confirms the supposition that you can *not* turn a space coordinate into a time one; it would thus not help you in the least, to interpret as real your imaginary symbolic transformation.

But perhaps it might be urged that Sophus Lie, who boasts himself to be "*ein Mathematiker von Fach*," has turned down Helmholtz' *Axiom der Monodromie* (as he insists on calling it), by showing that he can get on without it. But that only means that his own axioms, somehow or other, imply *Monodromie*. We may take it, if only for the sake of argument, that they do; for Sophus Lie is *ein Mathematiker von Fach*. But for our present purpose it would not matter if we were to take it further that Helmholtz' earlier three axioms (or rather hypotheses) also imply *Monodromie*, by some indirect deduction, not obvious to the layman; that might indeed give Lie an opportunity for criticizing Helmholtz as an amateur, but it would bring Lie in on his side, in an argument to show that a group which had not *Monodromie* must not be called a "space." Helmholtz in this case perhaps failed to realize all the symbolic consequences of his first three hypotheses; but a far graver criticism lies against Lie, if he wished his symbols to have real import; for Minkowski has shown that real import (as Time-Space) can be given to a group which has not *Monodromie*, and is not included in the Euclidean and two non-Euclidean groups which Lie discusses, and yet is a *Transformationsgruppe* in his sense of the term, and this is just what Helmholtz might almost be said to have anticipated.

But what then is the moral of all this? It has a very significant one, which can I think be appreciated even by us laymen. It is: to show, that what Einstein and Minkowski have done, is not to show that "Time is a fourth dimension of space." They have done exactly the opposite; they have refuted the old vague assertions to that effect, by showing that that is just what Time is not. The applicability of their symbolic Theory to time and space, the real import which can be given to it as Time-Space, depends essentially on its *not* being conceived as what Euclid, Helmholtz, and the man in the street, mean by "space." Helmholtz had the genius to catch on to the essential distinction between the two, before anybody had thought of a real illustration of a group without *Monodromie*. But even with this illustration before him, Dr. Robb fails to draw the right conclusion from his own symbols; which was not that Euclid had failed, but that he himself had not succeeded, in distinguishing between real and symbolic propositions.

Billy Dun, Half Way Tree, Jamaica. EDWARD T. DIXON.

GLEANINGS FAR AND NEAR.

708. Trumper scored in Test matches 17,150 runs at an average of 45·01 .... Facts, as Mr. Bounderly would say.—N. Cardus, *The Summer Game*, 1929.

709. It may not be generally known that the *Bougainvillea*, whose clusters of purple blooms are well known to travellers and residents in the East, takes its name from General Bougainville, who published (1754-6) his *Traité du Calcul Intégral* as a sequel to De L'Hôpital's *Analyse des Infiniment-Petits*.

AN EARLY METHOD FOR SUMMATION OF SERIES.

By T. A. A. Broadbent.

1. The method given by Euler * for the transformation of series was used by him to obtain "sums" for various divergent series, and recently Knopp † has written extensively on the "Euler summation process" derived from this transformation. But it does not seem to have been remarked that an interesting variation of this method is given by Hutton in the first volume of his *Tracts*, published in 1812. In Tract 8, "The Valuation of Infinite Series," written in 1780, Hutton gives an account of his method, claiming to have arrived at it independently of Euler's work, and employing it to approximate to the sums of series whose terms alternate in sign. When the proposed series converges, we have an easy and rapid way of determining its sum, and in the case of a divergent series, a number is determined which would nowadays be recognized as a conventional sum. Very little attention seems to have been paid to Hutton's paper and his name is apparently not mentioned by modern writers on this subject.‡ De Morgan § refers to the "remarkable method" of Hutton, but he applies it only to convergent series, and makes no reference to its application to oscillating series. This is noteworthy in view of the remarks made by De Morgan at the end of his Chapter 19 on "The Transformation of Divergent Developments," but it might be conjectured that even so acute a logician as De Morgan had not clearly perceived, at any rate at the time of writing his "Calculus," that the word "sum" applied to any infinite series is being used in a conventional sense.

It is not to be expected that Hutton, writing in 1780, had any precise notions on convergence. In Tract 7 on "The Nature and Value of Infinite Series," he defines a convergent series as one in which the absolute magnitudes of the terms form a decreasing sequence and a divergent series as one in which they form an increasing sequence; a series of the type

$$(1.1) \qquad a - a + a \ldots$$

is said to be neither convergent nor divergent but neutral. The theory given by Hutton turns on a fallacy connected with the use of this series. For this reason, although the theory is interesting and suggestive, possibly the most valuable part of his work is the neat and systematic setting-out of the computations.

2.1. A brief indication may here be given of the contents of the present note. In the following section (2.2) an account will be given of the elementary but fallacious considerations from which Hutton obtains an approximation to the sum of a series in terms of its first n terms, and an example of its use will be set out. In (3.1) we shall endeavour to provide a proof of Hutton's results from a consideration of the circumstances under which they are valid. The relation with the Euler series of differences will be indicated in (3.2), while in (3.3) the practical advantages of Hutton's work will be compared with those of Euler's procedure. Finally, in (4), we shall be concerned with applications to divergent series.

2.2. Starting with the neutral series

$$(2.21) \qquad a_0 - a_0 + a_0 \ldots,$$

the sum of this series is taken as $\frac{1}{2}a_0$, several more or less plausible reasons being advanced in support of this choice. Now consider the following series, in which the terms alternate in sign and decrease in absolute magnitude:

$$(2.22) \qquad a_0 - a_1 + a_2 \ldots.$$

* Euler, *Inst. Calc. Diff.*, II, cap. I (1755).

† Knopp, *Math. Zeit.*, 15 (1922), p. 226, 18 (1923), p. 125; *Infinite Series* (English translation, 1928), chap. 13.

‡ There is a short reference in a paper "Zur Geschichte der divergenten Reihen," by Burkhardt, *Math. Annalen*, 70 (1911), p. 170.

§ De Morgan, *Diff. and Int. Calculus* (1842), chap. 18, "On Interpolation and Summation."

The sum of this series must lie between 0 and a_0. Since the sum of the series (2.21) is $\frac{1}{2}a_0$, and since

$$a_0 - a_1 \geqslant a_0 - a_0, \quad a_2 - a_3 \geqslant a_0 - a_0, \ldots$$

the sum of the series (2.22) must be greater than th e sum of the series (2.21). Hence, if A denotes the sum of the series (2.22),

$$0 \leqslant \tfrac{1}{2}a_0 \leqslant A \leqslant a_0.$$

But if A_1 denotes the sum of the series

$$a_1 - a_2 + a_3 \ldots,$$

we must have A_1 greater than $\frac{1}{2}a_1$. Thus

$$a_0 - a_1 \leqslant A = a_0 - A_1 \leqslant a_0 - \tfrac{1}{2}a_1 \leqslant a_0.$$

Forming in this way the arithmetic means of the partial sums we arrive at the following table:

Sums	Means
$A \geqslant 0$;	$A \geqslant \frac{1}{2}a_0$.
$A \leqslant a_0$;	$A \leqslant a_0 - \frac{1}{2}a_1$.
$A \geqslant a_0 - a_1$;	$A \geqslant a_0 - a_1 + \frac{1}{2}a_2$.
$A \leqslant a_0 - a_1 + a_2$;	$A \leqslant a_0 - a_1 + a_2 - \frac{1}{2}a_3$, etc.

Each term in the column of means is thus a closer approximation to A than the corresponding term in the column of sums. We may now obtain from the column of means a column of second means formed from them as the means were formed from the column of sums, and then a column of third means and so on. Setting these out

(2.23)

	SUMS.	1ST MEANS.	2ND MEANS.	3RD MEANS.
$A \geqslant$	0	$\frac{1}{2}a_0$	$\frac{1}{4}(3a_0 - a_1)$	$\frac{1}{8}(7a_0 - 4a_1 + a_2)$
$A \leqslant$	a_0	$a_0 - \frac{1}{2}a_1$	$a_0 - \frac{1}{4}(3a_1 - a_2)$	$a_0 - \frac{1}{8}(7a_1 - 4a_2 + a_3)$
$A \geqslant$	$a_0 - a_1$	$a_0 - a_1 + \frac{1}{2}a_2$	$a_0 - a_1 + \frac{1}{4}(3a_2 - a_3)$	$a_0 - a_1 + \frac{1}{8}(7a_2 - 4a_3 + a_4)$
$A \leqslant$	$a_0 - a_1 + a_2$	$a_0 - a_1 + a_2 - \frac{1}{2}a_3$	$a_0 - a_1 + a_2 - \frac{1}{4}(3a_3 - a_4)$	$a_0 - a_1 + a_2 - \frac{1}{8}(7a_3 - 4a_4 + a_5)$

In any column the numbers are alternately greater and less than the sum A, in any row they are all greater or all less than A.

Since any first mean is a closer approximation than the corresponding sum, any second mean closer than the corresponding first mean, and so on, and since the sums give in their order increasingly closer approximations, it follows that we obtain better approximations as we pass along any row or down any column.

2.3. Since bracketing is not a legitimate process for the series (2.21), this train of reasoning is invalid. To test the conclusions, let us write out the table for a simple series of which we know the sum, say,

$$(2.31) \quad 1 - \tfrac{1}{2} + \tfrac{1}{3} - \tfrac{1}{4} \ldots.$$

We have

0	0·5	0·625	0·666666
1	0·75	0·708333	0·697916
0·5	0·666666	0·6875	0·691666
0·833333	0·708333	0·695833	0·693750

Since the sum of this series is $\log_e 2$, we see that the rows either increase or decrease towards this value, and the terms in any column are alternately

greater and less than this value and tend to it. Thus in this case Hutton's conclusions are verified.

But if we construct the table for the series

$$a_0=\frac{1}{4},\quad a_1=\frac{1}{4}-\frac{1}{4^2},\quad a_2=\frac{1}{6},\quad a_3=\frac{1}{6}-\frac{1}{6^2},\quad a_4=\frac{1}{8},\quad a_5=\frac{1}{8}-\frac{1}{8^2},\ldots,$$

then it is found that the various rows and columns do not obey the rules laid down by Hutton.

3.1. Let us take any convergent series of the type (2.22) and construct Hutton's table for this series. If this table is to possess the properties ascribed to it by Hutton, it is easily seen, by taking the differences of successive terms in a row or column, that we must have the following restriction on the terms:

$$(3.11)\qquad \Delta^n a_m \equiv a_m-\binom{n}{1}a_{m+1}+\binom{n}{2}a_{m+2}\ldots+(-)^n\binom{n}{n}a_{m+n}\geqslant 0,\ \text{all } m,\ n.$$

This is the condition which defines a totally monotonic sequence, to use Schur's term. We shall make use of a slight generalization and say that under these circumstances the sequence of terms is totally monotonic in the positive sense.

Assuming, then, that (3.11) holds, it is a trivial matter to show that the first row of the table is monotonic increasing, the second row monotonic decreasing, while every term in the second row is greater than the corresponding term in the first row. Hence every term in the second row is greater than any term in the first row. Thus these rows form bounded monotonic sequences and the same is true of every row. Every row, therefore, has a unique limit, r_n say, being the limit for the nth row, where

$$r_1\leqslant r_2,\quad r_3\leqslant r_4,\ \ldots.$$

But we also find, using (3.11), that each term of the third row is greater than the corresponding term of the first row, and so

$$r_1\leqslant r_3,\quad r_4\leqslant r_2,\ldots.$$

Combining these results

$$r_1\leqslant r_3\leqslant r_5\leqslant\ \ldots\ldots\leqslant r_6\leqslant r_4\leqslant r_2.$$

Now by a generalised form of a theorem on limits * first proved by Cauchy, if a_n tends to zero, then

$$\lim\left\{a_0-\binom{n}{1}a_1\ldots+(-)^n a_n\right\}\Big/2^n=0.$$

That is to say, the difference between the $(n+1)$th terms in the first and second rows tends to zero. Thus $r_1=r_2$, and so

$$r_1=r_2=r_3=r_4=\ldots.$$

But since r_{n+1} lies between $a_0-a_1+\ldots+(-)^n a_n$ and $a_0-a_1+\ldots+(-)^{n-1}a_{n-1}$, we have at once

$$\lim r_n=A.$$

Hence

$$r_1=r_2=r_3=\ldots=A.$$

It is easy to prove in like manner that each column is made up of terms taken alternately from two sequences, one monotonic increasing, the other monotonic decreasing, each having the same limit A, so that the column itself has a unique limit A.

Thus an approximation to the sum of the series (2.22) is

$$a_0-a_1+a_2\ldots\ldots+\tfrac{1}{2}(-)^n a_n,$$

a fact which seems to have been known to Newton, as a reference to a special case is to be found in his letter to Oldenburg dated 24th October, 1676.

* Knopp, *Infinite Series*, p. 75, ex. 8.

Taking the first row, we have the following theorem: For a convergent series in which the terms form a totally monotonic sequence, an increasing sequence whose limit is the sum of the series is given by

$$(3.12)\quad 0,\ \tfrac{1}{2}a_0,\ (3a_0-a_1)/4,\ (7a_0-4a_1+a_2)/8,\ \dots$$
$$(c_{n,0}a_0-c_{n,1}a_1\dots+(-)^r c_{n,r}a_r\dots+(-)^n c_{n\ n}a_n)/2^n,\ \dots,$$

where the $(n+1)$th term has a denominator 2^n and in the numerator

$$(3.121)\qquad c_{n,r}=c_{n,r-1}-\binom{n}{r},\quad c_{n,0}=(2^n-1).$$

Exhibiting the numerator coefficients in columns below that power of 2 which appears in the corresponding denominator, we have

(3.13)	2	2^2	2^3	2^4	2^5	2^6	2^7	2^8	2^9	2^{10}
	1	3	7	15	31	63	127	255	511	1023
		1	4	11	26	57	120	247	502	1013
			1	5	16	42	99	219	466	968
				1	6	22	64	163	382	848
					1	7	29	93	256	638
						1	8	37	130	386
							1	9	46	176
								1	10	56
									1	11
										1

where the numbers in the first row below the powers of two are one less than the corresponding denominators, and each other number is the sum of the two numbers in the preceding column which lie, one in the same row and the other in the preceding row.

3.2. The sequence (3.12) has a unique limit A. Hence if we consider the nth term of this sequence as the nth partial sum of an infinite series, that infinite series will converge to sum A. The nth term of this series will be the difference between the nth and the $(n+1)$th terms of the sequence and this difference is

$$\left\{a_0-\binom{n}{1}a_1\dots+(-)^n a_n\right\}\Big/2^{n+1}.$$

That is to say, the series

$$(3.21)\qquad \sum_{n=0}^{\infty}\Delta^n a_0/2^{n+1}$$

converges to sum A. This is Euler's series. Hutton notes this fact, without giving any precise proof.

It must be noted that we have only proved the convergence of Euler's series under the hypothesis of a totally monotonic sequence of terms in the original series. But it is known that the Euler series will converge whenever the original series does, a theorem first proved by Ames.*

Since the Euler series converges it follows that for a convergent series the first row of Hutton's table (2.23) always gives a sequence tending to the sum of the series, but in the general case it will not be a monotonic sequence. Since the condition for a totally monotonic sequence implies that the terms decrease more and more slowly, it is precisely in the case of a slowly convergent series of alternating terms that Hutton's process is valuable.

Further difference series can of course be obtained from other rows of the table (2.23).

3.3. In practice it is preferable not to use the sequence (3.12), but to set out the whole of the table as we did for the series (2.31). We write down in column a number of terms, then the column of successive sums, then the

* Ames, *Annals of Math.* (2), vol. 3 (1901), p. 185; Knopp, *Infinite Series*, chap. 8, p. 244.

column of first means, the column of second means, and so on. This may sound laborious, but an advantage lies in the fact that we can easily observe any break down in the monotonic property of the rows or in the alternating monotonic property of the columns. Such a breakdown indicates either that an arithmetical blunder has been made, or that the series is not amenable to Hutton's method. Since the second alternative is usually easy to settle from the appearance of the series, we have a valuable indication of an arithmetical slip if one should arise. The present writer can vouch for the value of such indications.

3.31. Take for example the series treated by Bromwich * as an example on Euler's transformation, the series

$$\sum_{1}^{\infty}(-)^n n^{-\frac{1}{2}}.$$

Beginning at the sixth term and changing the sign for convenience we readily obtain the table

40825	40825				
		21927			
−37796	03029		21316		
		20706		21264	
35355	38384		21212		21257
		21717		21250	
−33333	05051		21289		21255
		20862		21260	
31623	36674		21230		21257
		21598		21254	
−30151	06523		21277		
		20957			
28868	35391				

where in actual work we should drop any figures which run through a whole column. This gives for the sum of the series beyond the first five terms, 0·21256. Since the sum of the first five terms is 0·81746, we have as an approximation to the sum of the series

$$0{\cdot}81746 - 0{\cdot}21256 = 0{\cdot}60490.$$

If we take the seven terms beginning at the seventh, we finally obtain an approximation 0·60489. Hence by the alternating property of the columns the sum lies between 0·60491 and 0·60489.

4. Passing now to the consideration of divergent series, it may happen that if we construct a table of means for such a series it will contain some convergent rows or columns. If these have a common limit, this may be defined as a conventional sum of the series, since it will have been derived by a process which satisfies at least a limited consistency condition. Clearly, since we are concerned only with series whose terms alternate in sign, it is only necessary to consider those series in which the absolute magnitudes of the terms increase. For it will be necessary to restrict our inquiry to series whose terms obey a monotonic condition of some type, and if they decrease they must tend to a non-zero limit l, say, and the sum to be assigned to such a series will be the sum of a convergent series with the addition of $\frac{1}{2}l$, the sum of the series

$$l - l + l \ldots.$$

4.1. We shall not give an account here of Hutton's discussion of divergent series; it is on similar lines to the arguments of 2.2. We shall assume at once that the sequence of terms is eventually totally monotonic, *i.e.*

$$(4.11) \qquad \Delta^n a_m \geqslant 0, \text{ all } m, \text{ and } n \geqslant 2.$$

* Bromwich, *Infinite Series* (2nd edition, 1926), p. 63.

With this assumption, the terms of any row in Hutton's table form a monotonic sequence if we omit the first term. Thus leaving out of consideration the first column, the rows give sequences which are alternately monotonic increasing and monotonic decreasing. Also each term in the first row is less than the corresponding term in the second row, and so each of these rows has a unique limit. Hence the Euler series of differences is convergent, since the sequence in the first row is, with a trivial alteration of the first term, the sequence of partial sums of the Euler series. Thus the nth term of the Euler series tends to zero. That is,

$$(\Delta^n a_0)/2^{n+1} \to 0, \text{ and so, } (\Delta^n a_0)/2^n \to 0.$$

But this second expression is the difference between the nth terms in the first and second rows, and hence the limits of these rows are equal. In like manner, under these conditions, all the rows tend to the same limit, and it can easily be proved that the columns all tend to this limit, the terms in any column alternating above and below the limit. We can, therefore, define this limit to be the conventional sum of the series.

4.2. As an example, let us take the series

$$\frac{5}{4} - \frac{5.7}{4.6} + \frac{5.7.9}{4.6.8} \cdots . \tag{4.21}$$

Hutton calculates the sum of this series by his own method and checks the result by remarking that from other considerations we know that the sum of this series is $\frac{1}{8}(2+\sqrt{2})$. He probably arrived at this expression by an Abel limit process, since

$$\frac{2}{3}(1+x)^{-\frac{3}{2}} - \frac{2}{3} + x = \frac{5}{4}x^2 - \frac{5.7}{4.6}x^3 + \frac{5.7.9}{4.6.8}x^4 \cdots ,$$

and if in the left-hand side of this equation we put x equal to unity, we have the value stated,

$$\tfrac{1}{8}(2+\sqrt{2}) = 0{\cdot}56903559.$$

Setting out the table, using the first eight terms calculated to six places of decimals, we find

1·25	1·25						
		520833					
−1·458333	−·208333		566406				
		611980		8685			
1·640625	1·432292		570964		8970		
		529948		9255		21	
−1·804688	−·372396		567546		9072		32	
		605144		8889		43		35
1·955079	1·582683		570232		9015		38	
		535320		9141		33		
−2·094727	−·512044		568050		9050			
		600780		8959				
2·225647	1·713603		569868					
		538956						
−2·349294	−·635691							

This gives as the sum 0·569035. The theoretically equivalent Euler process applied to the same terms, gives 0·569029.

4.3. If the absolute magnitudes of the terms increase so rapidly as to violate the condition (4.11), the rows may not give a convergent sequence, and it is frequently the case that the first row will now form a sequence of terms of alternating sign, in which the absolute magnitudes of the terms increase less rapidly than in the column of partial sums. To this new sequence we may apply the process of taking repeated means of successive pairs of terms once

again, in the hope that this time we shall obtain a Hutton table with converging sequences. In this fashion Hutton arrives at a sum of the series

$$1! - 2! + 3! - 4! \ldots .$$

Euler had calculated the sum of this series by means of his series of differences, but his result is incorrect in the third place of decimals. Hutton obtains the value 0·403652; Borel * in his tract ascribes the first accurate computation of this series to Lacroix, and gives its sum as 0·4036526.... Hutton's work is dated 1780, and was published in 1812, seven years before the appearance of the second edition of Lacroix.

Readers of the *Gazette* will not be surprised to learn that the present writer owes his acquaintance with Hutton's tract entirely to the good offices and vast bibliographical knowledge of Professor E. H. Neville.

T. A. A. BROADBENT.

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710. *Squaring the Circle.*

A square and its inscribed circle have their areas in the proportion of their circumferences. This very simple theorem is very seldom stated. I recommend it to those who square the circle without knowledge of geometry; and if three of them, at least, do not get three different quadratures from it, they have ceased to be themselves, and have become fraudulent imitations.—De Morgan (*N. and Q.* III. vi. 67).

711. *Some New Thoughts founded upon New Principles*, by B. H. J., Lond., 1714, 4to. On the motion of the earth, tides, longitude, etc. Though published nearly thirty years after the *Principia*, and dedicated to the Royal Society, Newton is neither named nor alluded to. This is nothing but pure ignorance, as is obvious; and it illustrates my belief that until long after his death, Newton was very little known to the mass of the people, or more known as Master of the Mint than as a discoverer in Science. The writer was, as to knowledge, one of the mass.—De Morgan (*N. and Q.* II. iv. p. 241).

712. *Divination.*

The following piece of conjuring was communicated to me by a friend. It is so very simple to those who are fit to see the *rationale* that I shall not explain it, in order that the adepts may have the use of it. The person who is to be astonished is directed to think of one of the numbers 1, 2, ... 9 and put it by. He is then told to write down any number he pleases, no matter of how many figures, to write down a number made of the same figures in any other order, and to subtract one from the other. Suppose he thinks of 17629738, and proceed as follows:

17629738
93768172
76138434

He is then told to take the number of letters in his father's and mother's Christian names, and in the name of one of the apostles, and to add them together, to multiply this number by 4, the inverted number by 5, and to add to both of these put together the number he first thought of. Say, William Henry, Jane, Peter, 21 letters in all, 12 when inverted; 4 times 21 is 84, 5 times 12 is 60, and, 8 being the number thought of, 84, 60, 8, make 152. This 1, 5, 2 he is to mix up with the 7, 6, 1, etc., above in any order he pleases, and to give the list to the conjurer. Say he gives

31182457364.

All this he has done in private. The conjurer sees nothing but this list of figures, and tells him immediately that the figure he thought of was 8.—De Morgan (*N. and Q.* II. iv. 186).

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* Borel, *Leçons sur les Séries Divergentes* (1901), p. 56 and footnote; Lacroix, *Traité du Calcul* (1819), t. iii, p. 346.

THE PLANETARIUM AT BERLIN.

By J. H. Blacklock, M.A.

From the earliest times the movements of the heavenly bodies have excited the interest of the inhabitants of the earth. The ancients believed that the earth was a flat disc floating in a great ocean, and they thought the sun was a god who drove his chariot across the sky every day. During the night he travelled back to the east across the great ocean, but his light was shut off by a range of high mountains which existed in the northern part of the earth. At night the majority of stars were noticed to move across the sky as if they were all fixed in some spherical shell which revolved. Several stars, however, seemed to move irregularly and were called planets or wanderers. Seven of these bodies were known—Moon, Mercury, Venus, Sun, Mars, Jupiter—and it is probably to these seven bodies that we owe our week of seven days.

Pythagoras regarded the earth as a sphere poised in space, and this view was strongly supported by Aristotle, who used the arguments, current to-day, of the appearance of ships at sea and of the shadow of the earth as seen in eclipses of the moon. The followers of Pythagoras held that the circle was the perfect plane figure and the sphere the perfect solid. The motions of the heavenly bodies could be in perfect figures only, and it was at first believed that each planet was set in a crystal sphere which revolved and carried the planet with it. The sphere had to be of crystal to account for the visibility of other planets and stars through it. Outside the seven planetary spheres was a still larger one in which were set the stars. This was believed to turn all the others and was called the primum mobile. The whole system was supposed to produce in its revolution, for the few privileged to hear the music of the spheres, sounds as of some magnificent harmony. The Pythagoreans thought that Pythagoras was one of those who heard this heavenly music.

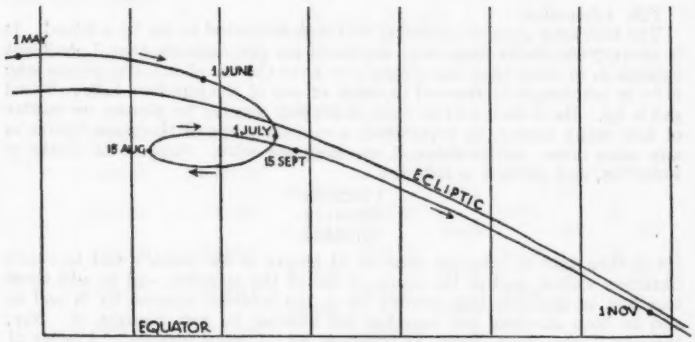

Fig. 1.

Careful observations of the movements of the planets showed that though they moved forward on the whole, they were sometimes stationary and sometimes went back. It was not to be supposed that the crystal spheres were irregular or that the idea of circular motion should be abandoned, so it was surmised that the main sphere carried not the planet itself, but the centre or axis of a subordinate sphere, and that the planet was carried on this. The main sphere would have to revolve once for a year of the particular planet, and the subsidiary sphere once for a year of the earth. Eudoxus (408-355 B.C.)

seems to have been one of the earliest teachers, if not the actual founder, of this theory of the universe.

Hipparchus (160-120 B.C.) was probably the greatest astronomer of ancient times. He improved upon the system of Eudoxus by introducing eccentrics as well as epicycles, and he it is to whom the credit of what is known as the Ptolemaic System is due. This system was given to the world by Ptolemy

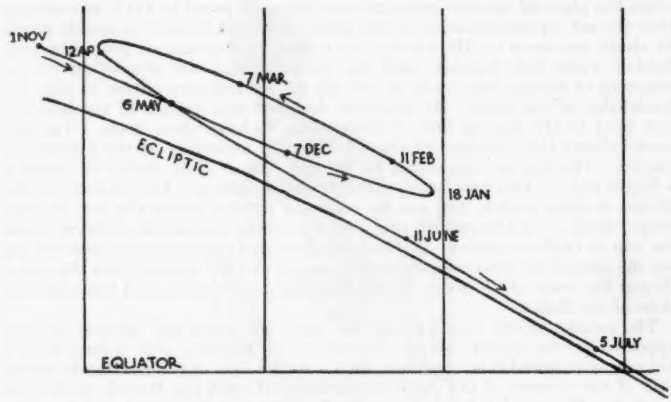

FIG. 2.

(*c.* A.D. 100-150) in his great work, *The Almagest* (140-150), and since it described fairly well the observed motions of the planets it was generally accepted until the sixteenth century.

In 1543 the great work of Copernicus (1473-1543) on the *Revolution of the Heavenly Bodies* was published. In this volume he taught that the sun was the centre of the solar system, and that the earth and the planets moved round it in circular orbits. As early as the third century B.C. Aristarchus of Samos had developed a genuine heliocentric system which was described by Archimedes and set aside with disapproval. The theory of Copernicus retained the idea of epicycles, and for a long time Kepler (1571-1630) worked on the assumption that the heavenly bodies moved in circles. His discovery that the orbits of the planets were ellipses with the sun at a focus did away with the need of epicycle and eccentric, and after him the work of Galileo (1564-1642) and Newton (1642-1727) developed and completed the Copernican system.

No teacher nowadays would think of teaching such a subject as geography, say, without using models. In this respect he is but following the example of great teachers of the past. Eudoxus, who, as I said before, was one of the forerunners of Ptolemy, constructed a model of the universe for which he required twenty-seven spheres. Archimedes, too, constructed an orrery which was taken to Rome after the fall of Syracuse and preserved as a curiosity for two or three hundred years. These models would no doubt be used in explaining to pupils the movements of the planets. When the Copernican theory began to be known, many men made models of the universe, which would work and show the earth and the planets going round the sun. In England Edward Wright (1560-1615) made one which could be used in predicting eclipses. It was shown as a curiosity in the Tower as late as 1675. The best known English model is probably that made to the order of Charles Boyle, Earl of Orrery, in 1715. It was an improved form of an

earlier planetarium invented by Geo. Graham, and showed the motions of the members of the solar system with relative sizes and distances, but with circular orbits, by means of arms and uprights moved by geared wheels. The word " orrery " for a planetarium was derived from this instrument.

The Zeiss Planetarium, a model of which has been erected in Berlin, is the latest attempt to show people the movements of the heavenly bodies. It owes its origin to Dr. von Miller, the Director of the Museum at Munich. Dr. Miller, when the plans of the new museum were being prepared in 1913, was anxious that the astronomical section of the museum should be used to enable people to study astronomy. He realised that the congregation of people in well-lighted cities, bad weather, and the thousand and one attractions or distractions of modern life, made it difficult for the ordinary citizen to gain any knowledge of the stars. He therefore designed two models of the heavens, and went to the famous firm of Zeiss, Jena, to have them made. The first model shows the members of the solar system according to the Copernican theory. The sun is represented by a large ball of glass, inside of which is a bright light. This ball is suspended from the ceiling. The planets are also shown as glass globes, and can be made to revolve round the sun at their proper rates. On the ground is a platform which carries the observer round the sun in twelve minutes. By looking through a periscope the observer can see the planets on their courses round the sun as they appear from the earth. Round the walls of the room, which is circular, are represented the constellations of the Zodiac.

The second model was intended to show the stars and planets as they appeared to the naked eye of an observer at Munich. For a long time it did not seem possible to construct such a model, but in the end the technical skill of the experts of the Jena firm triumphed, and the Munich model was finished. This model differs from the Zeiss Planetarium in that it is set to show the stars and planets as they appear to an observer in the latitude of Munich only. Before the Munich Planetarium was erected at Munich it was exhibited to the public in a specially constructed building on the roof of one of the Jena factories, and from August 1924 to January 1926 about 80,000 people visited it.

The aim of the Zeiss Planetarium is to show to an observer the appearance of the stars and planets in the sky at any point on the earth's surface. No fewer than 5400 fixed stars of the N. and S. hemispheres can be shown, and the various constellations can easily be picked out. More remarkable than these even is the Milky Way, which is reproduced in a most realistic fashion. The positions of the planets visible to the naked eye can be shown, and the waxing and waning of the moon can be seen as the moon performs its monthly revolution round the earth. For all this an artificial " sky " is needed, and the building which is intended for the housing of the Planetarium must have a hemispherical ceiling.

The building which contains the Berlin Planetarium is situated in a corner of the grounds of the Zoological Gardens, and is easily reached from all parts of the city by tram, train, or omnibus. From the outside it is not very imposing, and gives the impression that no money has been wasted on unnecessary ornament. The main hall is circular in plan and has a hemispherical ceiling covered with a white material. The diameter of the hall is 25 metres, and it is 15·5 metres high. There is seating accommodation for 420 people. There are besides an entrance hall and box office and cloakrooms. The arrangements for heating and ventilating the building are placed in a cellar under the entrance hall.

In the lecture room the instrument is placed at the centre, and the lecturer's desk has a switchboard from which the various motors and lighting devices are controlled. At the side is a lantern by means of which pictures can be thrown on to the ceiling. To indicate the position of any star of which he may

be speaking the lecturer uses a specially constructed electric torch which throws an illuminated arrow on the ceiling. Before lectures the room is lighted by two lamps on the Planetarium which are made in such a way that they can be turned out gradually. During the lectures the hall is illuminated by red lamps, much in the same way as picture palaces are lighted during performances.

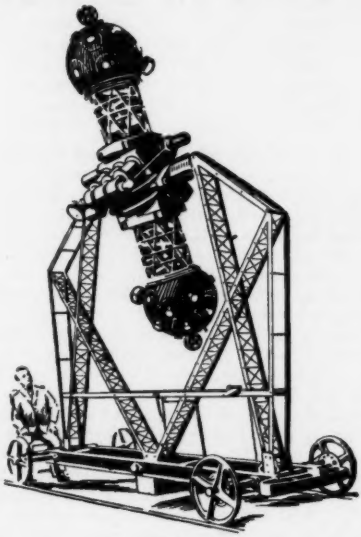

FIG. 3.

The Planetarium is fixed in the middle of the building so that the frame points E. and W. A section through the instrument along this line shows that the axis of the instrument is inclined to the vertical at an angle of 23½°, the upper end being tilted towards the E. The apparatus, as can be seen from the diagram, consists of a cylinder with two spheres at the end. The length over all is about 3·5 metres and the diameter of the cylinder about ·6 m. The weight of the instrument is 1800 Kg. It has no fewer than 120 lenses and seven motors. The lenses in the spheres show the fixed stars, those in the cylinder the Sun, Moon, and the planets Mercury, Venus, Mars, Jupiter, and Saturn. The Milky Way is shown by the special piece of apparatus which projects under the sphere. The projecting small sphere at the top shows how the end of the earth's axis produced to meet the celestial sphere describes a circle in the sky. There are other lenses which show the equator and the ecliptic. The motors are used for turning the whole instrument, and for producing the effect of a day in two different times, and also for making a round and year go by in 4 minutes, 1 minute, or 7 seconds.

When the lights have been turned down and the switch controlling the apparatus in the sphere has been touched, the dome looks exactly like the sky on a specially calm winter's night. All the constellations appear almost exactly as they do out of doors, and most wonderful of all, the Milky Way is seen in all its glory. If now the motors are set working, the stars move round and the day's journey is completed in 2 or 3 minutes.

If it is desired to watch the movements of any planet in the sky, it is easy to switch off all the stars and turn on the particular planet wanted, or if one prefers to see the planet moving among the stars it is quite a simple matter to arrange for that.

The phases of the moon can easily be shown, and the moon can be made to perform its monthly journey round the earth in three different times, according to the motor used. The " years " of the planets take the following times :

Earth - - -	4 min.
Mercury - -	58 secs.
Venus - - -	148 secs.
Mars - - -	7·2 min.
Jupiter - -	47·2 min.
Saturn - - -	1 hr. 56 min.

As I have said, the Zeiss Planetarium can show the stars not only as they are seen at Berlin, but as they are seen at any point on the earth's surface. By turning it upside down we can see the stars visible in the S. hemisphere. By setting it horizontally we can see the stars as they appear at the equator, and, generally, to see the stars as they appear in any particular latitude we simply have to set the instrument so that it makes the required angle.

There is also an arrangement by which the date can be read off. For instance, the lecturer can easily show us what the sky will look like on 11th May, 1935, or on any other date.

Every day a lecture is given during the morning to some of the children attending the schools in Berlin. In the evening three lectures are given, and they seem to be quite well attended. The one I attended was on " The Moon—Old Beliefs and Modern Knowledge." The lecturer showed slides of the moon at different phases, and had much to say of craters and mountains on the moon. Then by means of the Planetarium he showed us the moon waxing and waning.

The idea of bringing school children to the Planetarium is a very good one, and must lead in time to greater knowledge of the stars and planets among the citizens of Berlin. With the greater knowledge will come increased interest, and it may be that in the future some of the school children of to-day will do work in astronomy of first-rate importance.

J. H. BLACKLOCK.

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713. " 1806. August 4th. Sent to Lord Yarborough the head of Sir Isaac Nuton." From the memoranda on the back of a charcoal sketch by Nollekens. —J. T. Smith, *Nollekens and his Times*, 1929, p. 263.

714.

Qual' è il geometra che tutto s'affige
Per misurar lo cerchio e non ritrova,
Pensando, quel principio ond' egli indige ;
Tale ero io a quella vista nuova.

Dante, *Paradiso*, Cant. xxxiii. 133-136.

715. Fry, a Senior Wrangler at Oxford, was a batsman who, as I say elsewhere, played by the book of arithmetic.—N. Cardus, *The Summer Game*, 1929.

716. A subsidy was counted the fifth part of a man's estate ; and so fifty subsidies is five-and-forty times more than a man is worth.—Selden's *Table Talk*, " Subsidy."

717. In Peacock's *Headlong Hall*, c. vi., the philosophers are made to talk of " the precession of the equinoxes " where they mean the " variation of the obliquity." This mistake is enhanced by a setting of knowledge unusual in a novel ; thus it is said that Laplace " has demonstrated that the precession of the equinoxes is only a secular equation of a very long long period."—A. De Morgan (*N. and Q.* III. iv. 185).

SHUFFLING A PACK OF CARDS AND THE THEORY OF NUMBERS.

By F. C. Boon, B.A.

If a pack of playing cards is shuffled systematically and the operation of shuffling repeated exactly, then after a certain number of repetitions of the operation the original order of the pack will be restored.

The method of shuffling in ordinary card-games is a rough and ready interleaving of two parts of the pack. If this method were carried out in simple perfection, one half of the pack would be interleaved with the other, so that no two cards adjacent before the shuffling would be together after.

The following question was set to a number of boys as a problem in experimental arithmetic:

"Suppose a pack shuffled as follows: Hold the pack face downwards in the left hand; in the right hand take the top half of the pack and insert it into the lower half so that each right-hand card is above the corresponding left-hand card. After how many such shuffles would the pack return to its original order?"

They were advised to try the problem for packs of different numbers of cards.

A priori, one would expect that any numerical notation assigned to the cards would give rise to circulating groups of numbers, such as one finds in the case of recurring decimals.

Denote the positions occupied by the cards in their original order, beginning from the bottom as 0, 1, 2, 3, ... Then the following tables represent the positions occupied successively by the various cards in packs of (1) 52 cards, (2) 26 cards, (3) 12 cards.

The top and bottom cards are stationary.

Case I. 52 cards.

(*a*) 1, 2, 4, 8, 16, 32, 13, 26;
(*b*) 5, 10, 20, 40, 29, 7, 14, 28;
(*c*) 11, 22, 44, 37, 23, 46, 41, 31;
(*d*) 19, 38, 25, 50, 49, 47, 43, 35;
(*e*) 3, 6, 12, 24, 48, 45, 39, 27;
(*f*) 9, 18, 36, 21, 42, 33, 15, 30;
(*g*) 17, 34.

Case II. 26 cards.

(*a*) 1, 2, 4, 8, 16, 7, 14, 3, 6, 12, 24, 23, 21, 17, 9, 18, 11, 22, 19, 13;
(*b*) 5, 10, 20, 15.

Case III. 12 cards.

(*a*) 1, 2, 4, 8, 5, 10, 9, 7, 3, 6.

The numbers in group (*a*) Case I indicate that any card originally in one of the positions passes successively through the other positions and then returns to its original position, thus the 13th card occupies successively the positions 13, 26, 1, 2, 4, 8, 16, 32; and so on.

In Case I the cards which circulate are divided into six groups of 8 cards and one group of 2 cards. After 8 shuffles each card is back in its original position, the cards in group (*g*) circulating 4 times.

In Case II 20 shuffles are necessary.

In Case III 10 are necessary.

One property of recurring decimals has already become apparent in Case I, group (*a*). It is analogous to the fact that the digits 076923, in order, form the circulating period for six different fractions of denominator 13.

Taking the three cases we see a resemblance to the fact that there is apparently an irregular connection between the denominator of a fraction and the

number of digits in the circulating period of the decimal equivalent; there is a corresponding difficulty in predicting the number of shuffles for a pack of a given number of cards.

The complementary law of recurring decimals has also its counterpart in the shuffling problem. Thus if the circulating period corresponding to a prime denominator is of an even number of digits, *e.g.* ·076923, the first half are complementary with respect to 9 to the other half.

In groups (*e*), (*f*), (*g*) of Case I the numbers are complementary in the same way with respect to 51; in the groups of Case II they are complementary with respect to 25 and in Case III with respect to 11.

Moreover in the other groups in Case I the numbers of one group are complementary (with respect to 51) to the numbers of another group. Thus taking group (*c*) in the order 46, 41, 31, etc., these numbers in order are complementary to the numbers of group (*b*).

There is one apparent breakdown in the analogy between these groups and circulating decimal periods. All decimals corresponding to fractions of the same denominator have the same number of digits in the period whatever the numerator of the vulgar fraction.

In Case I the groups are not all composed of the same number of integers. Nor are they in Case II. This is to be accounted for by the fact that 51 in Case I (or 25 in Case II) is not prime.

Now in the case of a composite denominator, *e.g.* 51, such fractions as 12/51 and 34/51 would be converted to decimals only after they had been reduced to the forms 4/17, 2/3. But the number of digits in the periods for 4/17 and 2/3 would be factors of the number in the period for 13/51.

The General Case. Now let n be the number of cards in the pack. (In this discussion n will be taken as even. For a pack of an odd number the conclusions for a pack of the next higher even number will apply.)

For $n-1$ write m and call m the modulus of the pack; m is odd.

Let N_{n-1} or N_m be the number of shuffles required.

It will be seen that the pth card will occupy successively the positions p, $2p$, 2^2p, 2^3p and so on, so that when $2^r p \equiv p \pmod{m}$ the card is back in its original position. Then N_m is given by the lowest value of r for which $2^r - 1 \equiv 0 \pmod{m}$, just as, in recurring decimals, the number of digits in the period is the lowest value of r for which $10^r - 1$ is divisible by the denominator.

If m is prime, m is either r or a factor of r, and this is in accordance with Fermat's theorem.

In Case III (12 cards) m is 11 and is prime, therefore N_{11} is either 10 or a factor of 10. But $2^2 - 1$ and $2^5 - 1$ are not divisible by 11; $2^5 + 1$ is, and therefore $2^{10} - 1$ is. As only $n-2$ cards circulate in the case of a pack of n cards, all the 10 cards in this case form one circulating group. This is analogous to the decimal equivalents of fractions of denominator 7.

In a pack of 18 cards m is 17. Now $2^8 - 1 = 255 = 17 \times 15$. N_{17} is 8, and the 16 cards that circulate are composed of two groups of 8 cards. This is analogous to the case of fractions of denominator 13.

In Case I (52 cards) 51 is a composite number. We expect N_{51} to be a common multiple of N_{17} and N_3, *i.e.* of 8 and 2. We also find actual groups accounted for by N_{17} and N_3, for we can write 1/17, 2/17, 3/17, etc., as 3/51, 6/51, 9/51 ... and 1/3 and 2/3 as 17/51 and 34/51. These groups are (*e*) and (*f*) and (*g*).

The numbers in the other four groups are all prime to 51, and therefore N_{51} should be either the number of integers less than 51 and prime to it or a factor of that number.

The general composite case $m = a^\alpha, b^\beta, c^\gamma, \ldots$ is covered by the extension of Fermat's theorem, viz. that if x and m are two numbers prime to each other, and $\phi(m)$ is the number of integers, including unity, less than m and prime to it, then $x^{\phi(m)} - 1 \equiv 0 \pmod{m}$.

In the cases under consideration $x=2$ and is therefore prime to m, which is odd.

So that although the number of shuffles cannot be precisely predicted, the number of trials necessary to determine it is restricted, as N_m must be either $\phi(m)$ or a factor of $\phi(m)$.

Also $\phi(m)=m(1-a^{-1})(1-b^{-1})(1-c^{-1})\ldots$.

In Case I $\phi(m)$ is $51(1-1/3)(1-1/17)$, *i.e.* 32 and N_{51} is obtained by trying 2^2-1, 2^4-1, 2^8-1, $2^{16}-1$, $2^{32}-1$ for divisibility by 51.

The formula for $\phi(m)$ shows that N_a, N_b, etc., are factors of N_m.

Case II (26 cards) is accounted for in this general discussion.

Here $$\phi(m)=25(1-1/5)=20,$$

and all values of 2^r-1 are to be tried, r being a factor of 20, until divisibility by 25 is obtained. $r=20$ is the lowest value giving 1048575 for 2^r-1.

Now in this case since 5 is a factor of 25, we should expect a circulating group corresponding to N_5 or to the fractions 5/25, 10/25, 15/25, 20/25, a group of 4 cards. This is group (b).

The remaining 20 cards will form a single group, for:

Consider $m=a^p$, a being prime.

Then if q is N_a, q is given by the lowest value of r for which $2^r\equiv 1 \pmod a$;

$\therefore$ if any other power of $2\equiv 1 \pmod a$, then it must be 2^{qt} where t is an integer.

Now we have $2^q=ha+1$ where h is an integer;

$$\therefore\ 2^{qt}=(ha+1)^t$$
$$\equiv (hat+1)\pmod{a^2},$$

and if $2^{qt}\equiv 1 \pmod{a^2}$, then $hat\equiv 0 \pmod{a^2}$.

Now h is prime to a for otherwise $ha=ka^2$ and $2^r-1=ka^2$ which is impossible under the conditions; $\therefore$ the lowest value of t is a.

That is, q is the number of cards in a circulating group for modulus a,

aq „ „ „ „ „ „ „ a^2,

and similarly

a^2q „ „ „ „ „ „ „ a^3,

and so on; and if q is $a-1$, the number of cards circulating is

$$(a-1)+a(a-1)+a^2(a-1)+\ldots=a^p-1=m-1.$$

Thus in a pack of 28 cards, modulus 27 or 3^3, we get groups of 2, 6, 18 cards, making up the 26 which circulate.

In a pack of 8 cards, modulus 7, the 6 cards that circulate are made up of two groups of 3; so that in a pack of 50 cards, modulus 49 or 7^2, the cards that circulate are made up of 2 groups of 3 and 2 groups of 3×7 cards.

In many cases the number of trials can be still further restricted by applying the knowledge that N_{ab} must be a multiple of both N_a and N_b.

E.g. for a pack of 100 cards ($m=99$) $N_3=2$, $N_9=6$ and $N_{11}=10$; $\therefore$ N_{99} is either 30 (the L.C.M. of 6 and 10) or a multiple of 30.

The number of integers below 99 and prime to it is $99(2/3)(10/11)$, *i.e.* 60;

$\therefore$ N_{99} is either 60 or a factor of 60.

It is only necessary to try $2^{30}-1$ and $2^{60}-1$.

Instead of working out these powers, it is simpler to write down the sequence of powers of 2, subtracting 99 whenever a number higher than 99 is reached:

1, 2, 4, 8, 16, 32, 64, 29, 58, 17, 34, 68, 37, 74, 49, 98.

This gives $$2^{15}\equiv 98 \pmod{99},$$
$$2^{15}\equiv -1 \pmod{99};$$
$$\therefore\ 2^{30}\equiv 1 \pmod{99}.$$

Thus 30 shuffles are required.

The problem, set as an experimental exercise, was approached by experiment. Suitable moduli of which the 3 given cases are typical were chosen, followed by others of more general form, such as $3^2 . 5$, etc. The theorem of primes and the extension of Fermat's theorem quoted above, gradually emerged inductively.

Such a problem then seems a suitable introduction to the study of the Theory of Numbers. The first apparent lack of connection between the number of cards and the number of shuffles has an element of provocation, the analogy with recurring decimals and the entry of the G.P. introduces familiar connections, and important theorems begin to appear and invite an attempt to look for the formal proofs that are usually given in the bookwork of the Theory of Numbers.

Dulwich College. F. C. B.

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718. In a number of years equal to the number of times a rhinoceros's mother is as old as the rhinoceros, the rhinoceros's father will be as many times as old as the rhinoceros is years old now. The rhinoceros's mother is twice as old as the rhinoceros will be when the rhinoceros's father is twice as old as the rhinoceros will be when the rhinoceros's mother is less by the difference in ages between the mother and father than three times as old as the rhinoceros will be when the rhinoceros's father is one year less than twelve times as old as the rhinoceros is when the rhinoceros's mother is eight times the age of the rhinoceros.

When the rhinoceros is as old as the rhinoceros's mother will be when the difference in ages between the rhinoceros's father and the rhinoceros is less than the age of the rhinoceros's mother by twice the difference in ages between the rhinoceros's father and the rhinoceros's mother, the rhinoceros's mother will be five times as old as the rhinoceros will be when the rhinoceros's father is one year more than ten times as old as the rhinoceros is when the rhinoceros is less by four years than one-seventh of the combined ages of his father and mother.

FIND THEIR RESPECTIVE AGES.

(For the purposes of this problem, the rhinoceros may be considered to be immortal.) [*Per* A. S. E. Ackermann.]

719. I have heard persons argue as if a legacy duty of ten per cent., exacted at ten transitions, would exhaust the whole fund.—A De. Morgan (*N. and Q.* I. xi. 426).

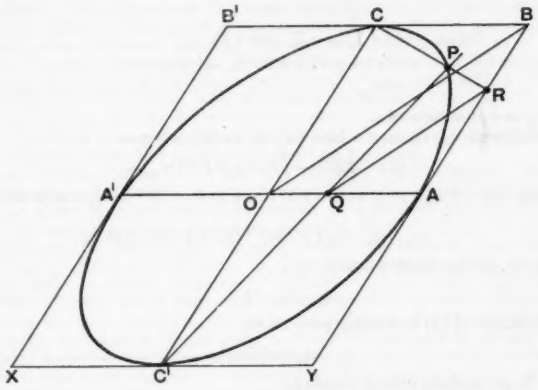
720. "They (the mathematicians) have a formula for everything; everything is classed, ticketed and in such a way as to preclude discussion; how can one dispute with a formula? . . . We are allowed to question the correctness of an application of grammatical rules or of an expression; the pupil may without any drawback criticise it, take it as doubtful, or hesitate before applying the rule; but we cannot imagine ourselves questioning the accuracy of a table of logarithms, or of the laws of gravity."—J. M. Guyan, *Education and Heredity*, p. 246 (1891). [*Per* Mr. Arnold Buxton.]

721. CROMWELL (Olivier) . . . Il n'y a guère d'exemples en Europe d'aucun homme qui, venu de si bas, se soit élevé si haut. . . . Fut-il heureux? il vécut pauvre et inquiet jusqu'à quarante trois ans; il se baigna depuis dans le sang, passa sa vie dans le trouble et mourut avant le temps à cinquante-sept ans. . . . Que l'on compare à cette vie celle de NEWTON qui a vécu quatre-vingt années, toujours tranquille, toujours honoré, toujours la lumière de tous les êtres pensants, voyant augmenter chaque jour sa renommée, sa réputation, sa fortune, sans avoir jamais ni soin, ni remords; et que l'on juge lequel a été le mieux partagé.

O curas hominum, o quantum est in rebus inane! (*Pers. Sat.* I. v. 1.) —Voltaire (*Dict. Philosophique*, article *Cromwell.*)

MATHEMATICAL NOTES.

950. [V. 1. a. 1.] *To inscribe the Principal Ellipse in a given Parallelogram.* Let $B'BXY$ be the parallelogram, and C, A, C', A' the mid-points of $B'B$, BY, YX, XB' respectively. AA', CC' intersect in O.



This method of setting out the problem given in works on Practical Geometry is objectionable.

To draughtsmen who seek accuracy and speed, the following simplification may be of interest, as it obviates many errors of manipulation:

Draw by means of set-squares lines such as QR parallel to OB. Then CR and $C'Q$ will meet in P a point on the required ellipse.

(i) If the above figure be a square, $BR/BA = QO/OA$ and $BR/BC = OQ/OC'$;

$$\therefore \hat{BCR} = \hat{CC'P}.$$

Hence by similar $\triangle$'s $\hat{CPC'} = 90°$ and P is a point on the inscribed circle.

On projection, BR/BA and OQ/OA remain the same, and the circle becomes an ellipse.

(ii) Using OA and OC as oblique axes:

Let $OA = a$, $OQ = t \,.\, a$, $AB = b$, $BR = t \,.\, b$, where t is any fraction.

Then the equation of $C'Q$ is

$$(y + b)\, t \,.\, a = bx. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

Then the equation of CR is

$$(y - b)\, a = -b \,.\, t \,.\, x. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2)$$

Eliminating t we get the locus of P as

$$(y^2 - b^2)\, a^2 = -b^2 x^2$$

or

$$x^2/a^2 + y^2/b^2 = 1.$$

Solving the equations of $C'Q$ and CR we get the coordinates of P as

$$x = 2\,at/(1 + t^2) \text{ and } y = b\,(1 - t^2)/(1 + t^2)$$

where $x = a \sin \phi$ and $y = b \cos \phi$ and $t = \tan \phi/2$.

BERTHA NAYLOR.

951. [D. 2. c.] *Stirling's Theorem.*

A consideration of Wallis's expression for $\sqrt{\pi/2}$ as an infinite product, viz.:

$$\sqrt{\pi/2} = \underset{n\to\infty}{\text{Lt.}}\ 2^{2n}(n!)^2/(2n!)\sqrt{2n} \quad \ldots\ldots(1)$$

shows that $n!$ for large values of n can be approximately determined when Lt. $2n!/n!$ is known.
$n\to\infty$

Now
$$\begin{aligned} 2n!/n! &= (n+1)(n+2)\ldots(n+n) \\ &= n^n(1+n^{-1})(1+2n^{-1})\ldots(1+n\,.\,n^{-1}) \\ &= n^n u_n, \quad \ldots\ldots(2) \end{aligned}$$

where u_n is a function of n.

To determine u_n, change n into $(n+1)$ in (2), whence

$$(2n+2)!/(n+1)! = (n+1)^{n+1}u_{n+1}.$$

Writing $(2n+2)!/(n+1)!$ as $(2n+2)(2n+1)(n+1)^{-1}(2n!)/n!$ and using (2), we have

$$u_{n+1}/u_n = 4(1+1/n)^{-n}(2n+1)/(2n+2)^{-1},$$

which gives, in the limit when $n=\infty$,

$$u_{n+1}/u_n = 4/e. \quad \ldots\ldots(3)$$

The solution of (3) is readily seen to be

$$u_n = a(4/e)^n, \quad \ldots\ldots(4)$$

where a is an undetermined constant.

To determine a, we have from (2) and (4)

$$(2n)!/n! = a\,.\,2^{2n}n^n e^{-n}, \quad \ldots\ldots(5)$$

whence, from (1),

$$n! = a\sqrt{n\pi}\ n^n e^{-n}. \quad \ldots\ldots(6)$$

Changing n into $2n$ and dividing by (6),

$$(2n!)/n! = \sqrt{2}\,.\,2^{2n}n^n e^{-n}. \quad \ldots\ldots(7)$$

By comparison of $(2n!)/n!$ in (5) and (7),

$$a = \sqrt{2}.$$

Hence, from (6),

$$n! = \sqrt{2n\pi}\ n^n e^{-n} \text{ when } n \text{ is very large.}$$

C. HARVEY.

952. [M¹. 3. i. a.] *The Involute of the Astroid.*

The following additional properties may be noted of this curve (*Math. Gazette*, May 1929), which is the envelope of a rod of fixed length $b\sin 2a$ sliding with its ends on two fixed lines inclined at an angle $2a$.

Suppose the rod makes an angle ϕ with a bisector of the two fixed lines.

Then the radius of curvature, $\rho = b(1+\cos^2 a - 3\sin^2\phi)$.

The perpendicular from the origin on the tangent,

$$p = \tfrac{1}{2}b(\cos 2\phi - \cos 2a).$$

The p, r equation is $r^2 + 3p^2 + 4bp\cos 2a - b^2\sin^2 2a = 0$.

The cartesian equation of the curve referred to the bisectors of the fixed lines as axes is given for the sake of completeness. It is

$$\begin{aligned} &(x^2+y^2)^3 - b^2(x^2+y^2)^2(1-\sin^2 a\cos^2 a) - 18b^2x^2y^2 \\ &\quad + 9b^2(x^4\sin^2 a + y^4\cos^2 a) - b^4(x^2-y^2)\cos 2a(8+\sin^2 2a) \\ &\quad + b^4(x^2+y^2)(8-5\sin^2 2a) - b^6\sin^4 2a = 0. \end{aligned}$$

H. V. MALLISON.

REVIEWS.

Lehrbuch der Funktionentheorie. By W. F. Osgood. Bd. I (5te Auflage), 1928, pp. xiv + 818, 44 goldmarks. Bd. II, 1te Lieferung (2te Auflage), pp. vii + 307, 18 goldmarks. 1929. (B. G. Teubner, Leipzig and Berlin.)

In this new edition of Osgood's book the general plan of the work has been left unaltered. The exposition has been modified in a few places and a little new matter added. The most material changes in Vol. I are a modified treatment of the Riemann integral, the addition (in Chapter 11) of a paragraph on Poincaré's theta-series and (in Chapter 14) of a theorem of the author's on sequences of harmonic functions, and a remodelling of certain parts of Chapter 14 dealing with Riemann surfaces. The part of Vol. II which comes under review deals with functions of several complex variables, and the principal change here is a generalization of Weierstrass's theorem giving conditions that such a function shall be rational.

The topics discussed in Vol. I are very largely of a "classical" character, and there was no occasion for drastic revision from the second edition of 1912. In the few cases in which the presentation of a subject has been seriously affected by recent research (as, for example, in the theory of conformal representation) the author has usually elected to keep to the traditional point of view. This procedure is in a general way not open to criticism since the older methods often have sufficient interest and importance to justify the existence of at least one easily accessible account of them. But we feel that the time has come for writers of books on the theory of functions of a complex variable to revise their attitude to one aspect, at any rate, of the subject, and to bring their presentation more into line with modern ideas. We refer to the part played in modern function theory by the "maximum modulus principle," that is to say, by the theorem that if $f(z)$ is regular in and on the boundary of a bounded region of the z-plane (and is not a constant) then the maximum of $|f(z)|$ occurs on the boundary of the region and not at an interior point. This theorem constitutes one of the most powerful weapons of argument in the theory of functions (as must be evident to readers of Landau's *Primzahlen* published twenty years ago), yet scarcely any allusion to it is to be found in the standard courses of analysis. The new methods in conformal representation have gone some way towards bringing about a more general recognition of the importance of the maximum modulus principle, but the tradition which regards the property in question as one to be mentioned casually (if at all) in connection with harmonic functions and promptly forgotten has not yet been broken down. There are two recent books which give excellent accounts of the subject, namely Hurwitz-Courant and Bieberbach; but little has been done by authors of older works to bring new editions up-to-date in this respect. In the work under review the subject does indeed receive a little attention, and the maximum modulus principle is to some extent recognized as having a status independent of the theory of harmonic functions. But the treatment is sketchy and confined to scattered observations. By way of proof we have on p. 315 (Vol. I) a proof for a circular region, on p. 559 a remark (not elaborated) that the general theorem follows easily from this special case, and on p. 654 an example in which the reader is asked to deduce the property in question from the corresponding property of harmonic functions; the book thus contains (so far as we have been able to discover) no general proof. Such applications as are to be found in the book are of a minor character. An excellent illustration of the utility of the maximum modulus principle is afforded by the development of the theory of integral functions subsequent to Weierstrass. This theory, which culminated in the epoch-making researches of Hadamard, has been so greatly simplified in recent years by the systematic use of the maximum modulus principle that its main results can now be presented in a very small compass. The inclusion of some account of this theory would have added considerably to the value of the book.

There are one or two minor features of the work which may prove a little irritating to the reader. One is the frequency of forward references and of

references to the examples which (excellent in themselves) are freely scattered through the book as exercises for the reader. There is also a tendency to dismiss as "obvious" or "easy to prove" theorems which do not seem to merit these descriptions. A proof of the theorem stated at the bottom of p. 159 (Vol. I), for example, would have been welcomed by the reviewer. Again the theorem that the circle of convergence of a power series has on its circumference at least one singularity of the function defined by the series should scarcely have been dismissed in two lines as "obvious" (p. 458).

It must not be inferred from the fact that we have chosen one or two points for criticism that our attitude to the book as a whole is unfavourable. This is far from being the case. The very real merits of the book are sufficiently well known to readers of previous editions, and there is no need to give a detailed account of them here. We may, however, mention one or two points. The treatment of zeros and poles by means of "Taylor's theorem with remainder" is to be commended, since it does not involve an unnecessary limiting process whose effect has subsequently to be cancelled by an appeal to the properties of power series. The proofs of the theorems on regularity of functions defined by limits (in particular by definite integrals) are rendered particularly simple by the use of the "converse of Cauchy's theorem." The general theory is freely illustrated by instructive examples, some incorporated in the text and some set as exercises for the reader. Finally, the book commends itself not only as a systematic account of the general properties of analytic functions, but also as a valuable source of information on a variety of special topics of which a satisfactory account is not easy to find elsewhere.

A. E. I.

Le théorème de Picard-Borel et la théorie des fonctions méromorphes. By ROLF NEVANLINNA. Pp. 171. 35 fr. 1929. Paris; Gauthier-Villars.

The theorem of Picard and its extensions form the subject-matter of a very large number of recent papers. Professor Rolf Nevanlinna has collected some of the most recent developments into the latest of the famous series of "Borel tracts." It is obviously a very important work. The author, of course, has made numerous original contributions to the subject. Perhaps one may hope that further research will simplify the methods as well as extend the results; but probably in the nature of things the subject cannot be made very easy. At any rate anyone who wants to know the latest about Picard's theorem will have to read this book. Let us attempt a short sketch of its contents.

The theory of integral functions is concerned in the first place with the relation between the order of the function (*i.e.* the order of $|f(z)|$ as $z \to \infty$, viz. lim. log log $M(r)$ / log r, where $M(r)$ is the maximum modulus of $|f(z)|$ on the circle $|z| = r$), and the distribution of the zeros of the function. If, instead of the zeros simply, we consider the points where $f(z)$ takes a given value a, we discover the important part played by the famous theorem of Picard, that there can be at most one value a which $f(z)$ does not take an infinity of times.

In this work we consider meromorphic functions instead of the more special integral functions. It is found possible to classify meromorphic functions by means of their "order," though naturally the process is more complicated than the corresponding one in the case of integral functions. It depends on the rate of increase of an auxiliary function $T(r)$, the definition of which is not very simple. This $T(r)$ plays much the same part in the theory of meromorphic functions as log $M(r)$ does in the theory of integral functions. For example (p. 40) if lim $T(r)/\log r$ is finite, then the meromorphic function reduces to a rational function.

This part of the work demands the closest attention; for example, the "second fundamental theorem" (p. 69) takes nearly a page to state, and is quite unintelligible apart from the preceding work.

However, all this highly technical introductory matter leads to some very interesting consequences. We can extend Picard's theorem to meromorphic functions, and in fact prove that a meromorphic function takes every value (including infinity), with *two* possible exceptions, an infinity of times. An integral function has "infinity" for an exceptional value which it does not

take; so, as a particular case of the general theorem, we find that an integral function has at most a finite exceptional value, and so obtain Picard's original theorem.

Another important idea which emerges from the work is the connection between the exceptional values of a function, *i.e.* the values which it does not take, or takes comparatively rarely, and its asymptotic values, *i.e.* the values to which it tends as $z \to \infty$ in particular ways. In general, the exceptional values and the asymptotic values are the same, though the exact result is not quite so simple as this. An elementary example is the function e^z, which has the values zero and infinity both as exceptional and as asymptotic values. We may say roughly that a function which omits a certain value, or takes it only very rarely, makes up for this negligence by tending to this value at infinity as rapidly as it can.

Several further applications of these ideas are noteworthy. For example, if two meromorphic functions $f_1(z)$ and $f_2(z)$ are such that, for five different values of a, the equations

$$f_1(z) = a, \quad f_2(z) = a,$$

are satisfied for the same values of z, then the two functions are identical.

There are a number of problems connected with these points which still require further investigation. The subject is not one which has made much appeal in the past to English writers. A serious attempt to master this work and some of the memoirs referred to in the bibliography could not be undertaken lightly, but we think that it would be well worth while.

E. C. TITCHMARSH.

An Introduction to the Geometry of n Dimensions. By D. M. Y. SOMMERVILLE. Pp. xvii + 196. 10s. net. 1929. (Methuen.)

A hundred years ago, nobody admitted the idea of geometry in more than three dimensions. But nowadays, every student who specializes in geometry is expected to work in n dimensions as a matter of course. The idea is no more scorned as absurd than is a complex number. Now Sommerville's new book most opportunely supplies such a student with the necessary groundwork, and is so readable that he becomes familiar with the hyperspaces with a minimum of effort—and without having to learn Italian.

The number of branches of geometry to which we are introduced is quite remarkable. These include incidence theorems, enumerative geometry, distances and angles, rational normal varieties and Plücker-Grassmann coordinates, all in the first 95 pages. Thereafter, the author is chiefly concerned with polytopes, on which he is a great authority.

He devotes a whole chapter to Euler's Theorem (about the numbers of vertices, edges and faces of a polyhedron) and its various extensions. For the ordinary three-dimensional theorem, he gives four different proofs, of which the most ingenious is surely that of von Staudt. The proof of the extension to n dimensions is that given by Schouto in the second volume of his *Mehrdimensionale Geometrie*, but it appears to contain two unjustifiable assumptions. The reference to Poincaré's elaborate proof is, however, given at the end of the chapter.

The particular polytopes under consideration are: frusta and sections of simplexes, generalizations of the prism and pyramid, and the ordinary and hyperbolic *regular* polytopes. Following Schläfli, a regular polytope in Euclidean space of $n+1$ dimensions, or (more generally) a regular "honeycomb" filling non-Euclidean space of n dimensions, is represented by an ordered set of n integers, all greater than 2. The idea of *hyperbolic* honeycombs (such as the polyhedron "37," bounded by an infinite number of equilateral triangles, occurring seven at each vertex and filling the hyperbolic plane) was first mentioned by Sommerville himself, in the *Palermo Rendiconti*.

One printer's error has been pointed out to me: the fourth line of page 19 should read "$(p+1)(n-p)$ given $(n-p-1)$-flats is. . . ."

A special feature of the book is the extensive bibliography given at the end of each chapter, enabling the reader, after his interest has been aroused (as it surely will be), to go deeper into whichever of the many fields he finds the most congenial.

H. S. M. COXETER.

Les Equations linéaires aux différences finies. By N. E. NÖRLUND. Pp. 153. 50 fr. 1929. (Gauthier-Villars.)

Finite-difference equations, as they occur in Mathematics, are so often to all appearances algebraic in their nature and application, that the more old-fashioned among us might be pardoned surprise at the inclusion of a work on finite differences in a series devoted to the theory of functions. The wider functional point of view, however, brings important advantages. We are no longer required to accept merely formal solutions without inquiry into their validity (if any): we can, of course, discuss equations of more general type. But most importantly the analogy between finite-difference equations and differential equations can be thrown into high relief. This possibility M. Nörlund keeps prominently in mind, and his work on that account may be read with interest by students of the theory of differential equations—and with satisfaction as well as interest—for it cannot be denied that differential equations come well out of the comparison, and we can congratulate ourselves that the world is not yet so far quantised that the differential equation must go the way of the black rat and the red squirrel, ousted by less pleasant neighbours.

M. Nörlund considers linear difference-equations of any order in a single variable and the equivalent *system* of equations of the first order in a single variable. The nature of the general solution of such equations is studied with care, and the author passes on to the solution of those types of difference-equation which are analogous to the Fuchs-Frobenius solution of differential equations. Here he relies on an earlier monograph, by himself in the same series, *Leçons sur les Séries d'interpolation*, and on work by G. D. Birkhoff. The treatment, it need scarcely be said, lacks nothing in thoroughness, but by its condensation does not make for easy reading on the first occasion. This is the more so since the burden of the analysis falls on suffixes and indices. The French italic fount is never too large at the best of times, and it can be understood that those pages in which we descend to the second order of small symbols are not for bedtime reading. These minor defects admitted, the work makes a scholarly and substantial contribution to the accessible literature on the theory of finite differences. T. CHAUNDY.

Elementary Differential Equations. By THORNTON C. FRY. Pp. x + 255. 10s. 6d. 1929. (Macmillan.)

The author of this book is a member of the technical staff of the Bell Telephone Laboratories, and his name is familiar to us from his excellent treatise on *Probability and its Engineering Applications*. Both books have evolved from lecture notes prepared originally for the members of the staff of the Telephone Laboratories. They will appeal primarily to students of electrical engineering, but may well prove of interest to a much wider circle.

In producing a text-book on Differential Equations in which the applications to pure and applied science are given special prominence, Dr. Fry, as he clearly recognizes, is undertaking a difficult task. There is always the danger that the applications may require the use of physical laws of which the student is ignorant. In that case the differential equation is not made any easier, but considerably harder, for difficulties, like armies, are most easily overcome when they are isolated. Some authors feel this so strongly that they insist on stating all problems in such a way that they appear as pieces of pure mathematics, and then, following the solution obtained without any reference to physics, they add a note explaining the physical meaning of the question and of its solution. This method is probably the best for a book that is expected to appeal to students of several different types, mathematical, physical, chemical and engineering. In a book like that before us, intended for a specialized class of readers, it is possible to bring the physical meaning more into prominence. The discussion in Chap. VII of the electrical significance of the Complementary Function and Particular Integral will "stimulate a lively interest in, and add depth of meaning to, abstract mathematical ideas which otherwise might remain rather vague." The electrical applications strike even a reader whose chief interests lie elsewhere as being the best feature of the book. However, Dr. Fry's applications are not

restricted to electricity. In Chap. III (The Origin of Differential Equations) and Chap. VI (Practical Applications of Differential Equations), he discusses mass action, the catenary, electric circuits, hydrodynamics, the conduction of heat, curvature, trajectories, falling bodies, bending of a beam, vibrating strings and drumheads, the brachistochrone, geodesics, and the application of the theory of probability to telephone problems. In these chapters the difficulties mentioned above become apparent, and the student is liable to be bewildered by physical terms which are new to him. The author says, "Some of these examples require the use of physical laws with which the student may not be familiar. . . . From our standpoint the laws are needed for only one purpose: to show that the differential equations follow naturally and directly from them—so directly, in fact, that were the student familiar with the subjects with which they deal he would find little difficulty in formulating the same equations for himself."

There is considerable difference of opinion concerning the best method of dealing with a differential equation which cannot be solved in finite terms by ordinary mathematical processes. Most mathematicians and physicists prefer numerical methods, such as Adams' or Runge's, while others find graphical methods sufficiently accurate. Dr. Fry evidently relies upon the Integraph, and says that "institutions where many differential equations are met, so that difficult integrations are frequent, usually possess such devices." (The cost, which is rather high, would of course not matter to a large company, but would be prohibitive in many educational institutions.) He also describes a graphical method which can be carried out with only the usual drawing instruments. No details of any numerical method are given, which, in a book of this kind, appears tantamount to denying their value for practical use. It would be interesting to have the opinion of other large industrial laboratories on this subject. The equations for which Dr. Fry uses the integraph or a graphical method appear to be of a type much simpler than those for which numerical methods are generally adopted.

The scope of the book is rather narrower than usual. It is confined to ordinary differential equations, and deals chiefly with those of the first and second order, particularly linear equations with constant coefficients. However, there is some discussion of singular solutions, and other geometrical matter. Chap. II, which explains the geometrical significance of differential equations of the first and second order, has some most interesting diagrams, which might appear with advantage in the usual text-books. It should be mentioned that the physical applications mostly occur in separate chapters, so that more than half the chapters are purely mathematical.

Some of the phrases in Dr. Fry's book are of a breezier type than is usual in books produced on this side of the Atlantic. Operational methods are illustrated by the argument of the Lord High Executioner in *The Mikado*, while a problem in the Calculus of Variations is headed *The Problem of Dido*. A digression into history of that ill-fated queen concludes as follows: "Later, she committed suicide in a very spectacular fashion to avoid marrying the same Iarbas: but that part of the story is not proper material for a text on differential equations." Some readers may think that the theological flavour of Ex. 5 (p. 145) is also not proper material, but all will agree that "mathematical results have an almost annoyingly monotonous habit of being sensible." In commending Dr. Fry's book to those interested in a treatment of an unusual type, we conclude with the warning that "the solution of differential equations is no fit occupation for the obtuse."

H. T. H. PIAGGIO.

Determinaciones de Tiempo y Latitud con el Altazimut Gautier y el Telescopio Zenital Wanschaff del Observatorio astronomico de La Plata. Por el Ing. civil JOSÉ ARAMBURO, Professor auxiliar de Geodesia y de Mecanica. La Plata (Rep. Argentina). 4 dollars. 1929.

The book is a full description of a series of observations for the determination of time and latitude made with two different instruments at the La Plata Observatory in 1917. The altazimuth is an instrument, as its name implies, which has freedom of motion both in a vertical and a horizontal

direction, turning about two axes at right angles to each other. Its position in each direction is given by graduated circles, which are read by microscopes. In the present series of observations it seems to have been used entirely in the meridian, so that it was practically a reversible transit circle. Its azimuth, or the small angular deviation from the true meridian, was obtained by observing stars near the pole, the error of the clock being found from stars in the neighbourhood of the equator. The clock used was made by Riefler, and worked in an airtight case, partially exhausted of air; its beats are recorded electrically on a chronograph.

The Wanschaff Zenith Telescope is of the kind employed for determining latitude by the Talcott method, which uses pairs of stars at about the same distance from the zenith. The volume gives full details of the observations made with both instruments, and of all the steps of their reduction. To those who are not already acquainted with the methods of modern astronomy of position, it will give a good idea of the numerous precautions that are taken to secure accuracy, and of the wonderfully high standard of accuracy that is attainable. When long series of observations are used, this approaches, though it does not quite reach, one hundredth of a second of arc; that is, the angular width of a thumb-nail about two hundred miles away.

A. C. D. CROMMELIN.

Calculus for Technical Students. By S. N. FORREST. Pp. 208, with appendix, tables and answers. 5s. 1929. (Arnold.)

There is a remarkable amount of good material packed away between the covers of this little volume. The first 128 pages lead the student very gradually, *viâ* Coordinate Geometry and Gradients, to the differentiation of powers of the independent variable, integration of these powers, where integration is first defined as the operation inverse to differentiation and afterwards shown to be equivalent to a quadrature; the whole being sufficient to enable the student to work numerous examples of applications to physical and geometrical problems. This way of attack, of course, necessitates that the proof of $D(x^n)=nx^{n-1}$ should be given by the Binomial Theorem for a positive index only. The second part, consisting of 80 pages only, is overcrowded, working up to the solution of the differential equation of damped harmonic motion (unforced).

It may be that, on account of the endeavour to get so much into so little, the author at times descends from rigour to something not far removed from plausibility. This is a pity; for if the technical student does not get his ideas of rigour in mathematics from his mathematical text-book, he will not get it anywhere else. Thus, while the author states "a negative increment of y corresponds to a positive increment in x, and the average gradient is therefore negative," he does not insist on the fact that the gradient is always measured *with increasing* x, *i.e.* the angle whose tangent is the gradient is an angle lying between $-90°$ and $+90°$. In fact, in discussing the condition of perpendicularity he says: "If ϕ is the angle made by *one* of the lines with the x-axis, the angle made by the other is $\phi+90°$, instead of $\phi\pm90°$"; and later he says that "we may, if we please, take the obtuse angle PSQ as the angle ϕ'," instead of the negative angle. Again, although the idea of a limit is very clearly introduced, the author ends with a definition which is not all that it should be; by using the word "assignable" instead of "assigned," he certainly shortens the definition, but he does not bring out the facts (i) that ϵ being *previously* assigned, no matter how small ϵ may be, the difference between $f(x)$ and $f(a)$ can be made less than ϵ, by taking the difference between x and a less than some other number η, and (ii) the difference between $f(x)$ and $f(a)$ will *remain less* than ϵ if the difference between x and a is made still smaller. There is no attempt to give even an incomplete proof of the four fundamental laws of limits, these being used as self-evident; thus, in the rule for the differentiation of a function of a function a plausible statement is given instead of a proof; while $dy/dx=1/(dx/dy)$ is left for the student to "puzzle out" as a relation between gradients and to compare it with the equation $m_1m_2=-1$. Continuity is considered only as graphical continuity; *e.g.* $1/(2-x)$ is stated to be discontinuous at $x=2$, only because there is a "jump

from $+\infty$ to $-\infty$," and no mention is made of the fact that it is discontinuous because it has no definite value for $x=2$.

In the second part, the introduction to the exponential *curve* is very neat, though as an introduction to the exponential *function* it is hardly sound; if this proof, Isaac Barrow's original proof, is given, it should have been followed out as Barrow did it, logically. Another instance of this rigorous method followed by hardly rigorous treatment occurs in connection with the proof, or justification, of $dy=f'(x)dx$, which is later used without comment in examples on integration by substitution.

It is suggested that the phrase "absolutely equal" should be altered to "exactly equal," since -3, according to mathematical usage is absolutely equal to $+3$; and, if the student afterwards proceeds to higher mathematics, he will be unnecessarily confused at being asked to prove that "an absolutely convergent series is convergent."

J. M. C.

Army Mathematics. Part I. By C. JENNINGS. 60 pages. Part II. By R. L. W. TOBUTT. Pp. vii + 224. 6s. 6d. 1928. (Oxford University Press, Humphrey Milford.)

Each part of this book has its own preface, index, and set of answers. Part I covers the syllabus for the Army Second Class Certificate, and has chapters on decimals, the Metric System, weights and measures, H.C.F., fractions, ratio, accounts and scale drawing.

The author brushes aside the teacher's routine difficulties, and sets about his task in a breezy sort of way. But, as one expects in a book of this kind, there is some looseness of expression. For example, in speaking of the number 123·45, the author says—"*the value 4 represents, therefore, ten times less than a unit, or one-tenth, written* $\frac{1}{10}$," while he introduces division from the expression, "*2 horses* $\times 2 = 4$ *horses*," by asking us to "*consider the above the other way round.*"

Equally scant regard is paid to the need of scientific accuracy. Thus, with regard to a recurring decimal, we are told that "*for practical purposes it is not necessary to take it beyond 3 places.*" We are also informed that "*we cannot add 2 horses and 3 cows without reducing them to the same denomination,*" although, we surmise, there are some respects in which they "add up" to 5 animals (without *any* reduction!).

The examples are of a practical nature, and such as would occur in Army routine. We are, however, intrigued by the entries in the accounts of the Regimental Concert Party (p. 39):

"*To donation from P.R.I.* - - - £5.
By refund to P.R.I. - - - - - £5."

Part II meets the needs of the Army First Class Certificate, and has chapters on Algebraical symbols and equations, the simple properties of the parallelogram, triangle and circle, scale drawing, similar figures, and graphs. There is also a set of test papers at the end.

At the outset the author gives a list of "materials required," the first of which is a "*well sharpened pencil,*" but he realises that other things are required as well, for he adds at the end—"*the will to succeed.*"

There is a curious allusion on p. 14, where it is stated that "*A man who has thoroughly grasped the elementary principles of Algebra, has, in fact, a trump card up his sleeve.*"

The authors point out in the Preface that the book is intended for those who cannot attend classes regularly.

Candidates for the Army Certificates have to seize any odd moment as it comes along, and will find this book eminently suited to their needs.

The authors are to be congratulated on a well thought out and brightly written book.

Surveying, FOR AGRICULTURAL STUDENTS, LAND AGENTS AND FARMERS. By A. H. HAINES. New Edition, 1929. Pp. 210. 173 diagrams. 12s. 6d. net. (Longmans, Green and Co. Ltd.)

The first chapter, on Mensuration, contains much useful information. We learn from it, among other things, that a load of new hay weighs 19 cwt.

1 qr. 4 lb. But we do not like the author's definition of a sphere—a "*solid circle*" !

Then follow, in the order shown, chapters on, Surveying with the chain—Scales, the Planimeter (6 pages)—Plan drawing, the Pantograph (11 pages)—Subdivision of areas—Levelling—Logarithms—Solution of triangles—the Box Sextant—Plane Table—Theodolite—Tacheometry.

The descriptions of the Planimeter and Pantograph are such as appear in the trade catalogues, and could be shortened without loss of effect. It is not easy to see on what grounds the author bases his assertion (p. 64) that the Amsler Planimeter "*gives more accurate results than those obtained by graphical methods*," or, that "*the theory of the instrument is fundamental in the twelfth and thirteenth propositions of the second book of Euclid.*"

The book is written on practical lines and might be useful to those who are taking an organised course in practical surveying. V. NAYLOR.

Studies in the History of Statistical Method. (Walker.)

The writer of this book describes the historical development of Statistical theory with special emphasis upon the work of the pioneers in the subject. The most valuable parts of it are the bibliographies and the foot-notes it contains. These, if we include the list which is presented of certain technical terms used in Statistics with references to their origin, occupy about one-fourth of the book. Anyone who proposes to engage upon statistical research in the theoretical field would be well advised first to study such a work as this. Again and again experience has proved that individuals have laboured long with problems that have already been solved by others, sometimes years before. One has only to read Dr. Walker's chapter on the Normal Curve to realise that this has been as true of statisticians as it is of other scientists.

This early chapter is the longest in the book. It is followed by two short ones on Moments and Percentiles and by one slightly fuller on Correlation. The English reader will not perhaps be greatly interested in the account given of Statistics as a subject of instruction in American Universities, but he should at least be impressed by its prevalence and prominence in America as compared with England. It needs no prophet to foresee a much bigger future for it in our country when we wake up to its importance.

It would not be fitting to close this short review without a word of praise for the excellent photographs which add considerably to the interest of the book. No doubt also they add to its expense, but the price seems high for a work of less than 250 pages. D. CARADOG JONES.

Lower and Middle Form Geometry. By F. W. WESTAWAY. Pp. xi + 260. 4s. 6d. 1928. (Blackie.)

This book contains the *Geometry for Preparatory Schools* (already noticed in these pages), together with some additional matter dealing mainly with similar figures and with the representation and mensuration of solids. W. J. D.

Stage A Geometry. By R. W. M. GIBBS. Pp. viii + 109. 2s. 1927. (A. & C. Black.)

The 1925 *Report of the Mathematical Association on the Teaching of Geometry* recommends an "Experimental" Stage A as an essential preparation for a "Deductive" Stage B; and this little volume of ten chapters is intended as a text-book for the use of beginners within the limits of Stage A, the subject-matter being carefully arranged to avoid the "danger of its becoming desultory and aimless." The young pupil is taught the use of his hands, his eyes and his reasoning faculties by drawing, folding, measuring, map-reading, field-work, the construction of card models, the tracking out the path of a moving point. For work out of doors only simple home-made apparatus is required, and directions for making such apparatus are given in the text. From each chapter a certain number of fundamental facts emerge, and these are collected together at the end of the chapter under the heading "The Treasury of Facts." The book is attractively illustrated with plenty of diagrams and practical examples, and serves its purpose admirably.

W. J. D.

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An outburst against the theory of relativity.

From Sir **T. Muir,** offprints of papers of his own, together with:

Communications de la Société mathématique de Kharkow. Sér. 2, Tomes 1-8 - - - - - - - - - - - - - 1888-1904
Can any member supplement this run?

From Prof. **E. H. Neville**:

O. GREGORY — Mathematics for Practical Men (2) - - - - 1833
"Being a Common-place Book of Principles, Theorems, Rules, and Tables, in various departments of Pure and Mixed Mathematics, with their Applications; especially to the pursuits of Surveyors, Architects, Mechanics, and Civil Engineers."

Taschenbuch für Mathematiker und Physiker. 2 Jahrgang - - 1911
The first of these Pocketbooks is in the Library. Later volumes would be welcome.

From Prof. **H. T. H. Piaggio** and from Prof. **D. K. Picken,** offprints of papers.

From Mr. **F. Underwood**:

R. SIMSON — Euclid's *Elements* and *Data* (18) - - - - - 1818
This edition includes a note on Logarithms by A. Robertson.

The following have been bought:

H. HANCOCK — Elliptic Integrals - - - Merriman-Woodward 18 1917

W. H. SMYTH — Cycle of Celestial Objects (2 vols.) - - - - - 1844
The sequel to these volumes was given to the Library three years ago, and the set is now complete.

P. W. WOOD — Twisted Cubic - - - - - Cambridge 14 1913
If any member will give No. 11 (Watson), the set will be complete as far as No. 14, but the later numbers, of which only No. 17 and No. 19 are here, are much to be desired.

ERRATA IN THE SECOND LIBRARY LIST.

The *Arithmetic* which figures under COCKER (p. 13), is the 'Vulgar' Arithmetic, and is not another edition of the *Decimal Arithmetick* recorded in List 1, but the word should end in a *k*.

The name HASTLER (pp. 22 and 48), though copied from a source which most Bibliographers accept as infallible, is wrong; on the title page of a later edition of the booklet, the author's name is given as HUSTLER.

For H. J. POINCARÉ (p. 34) *read* J. H. POINCARÉ.

Under STEINER (p. 40), *for* mittalst *read* mittelst.

Under TARRY (p. 41), it should be said that the title *Théorie des tablettes* . . . is a correction of a misleading title in List 1.

BUREAU FOR THE SOLUTION OF PROBLEMS.

THIS is under the direction of Mr. A. S. Gosset Tanner, M.A., Derby School, Derby, to whom all enquiries should be addressed, accompanied by a stamped and addressed envelope for the reply. Applicants, who must be members of the Mathematical Association, should wherever possible state the source of their problems and the names and authors of the text-books on the subject which they possess. As a general rule the questions submitted should not be beyond the standard of University Scholarship Examinations. The names of those sending the questions will not be published.

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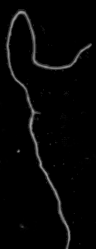
722. (Prof. J. B. Bury) was classical, philosophical and mathematical. He used to declare that his cure for headache was to read a stiff mathematical work, and this cure he used to the end of his life.

723. "The solution of the annuity problem, if it will be of any use, you have my leave to insert it into the *Philosophical Transactions*, so it be without my name to it. For I see not what there is desirable in public esteem, were I able to acquire and maintain it. It would, perhaps, increase my acquaintance, the thing which I chiefly study to decline. Of that problem I could give exacter solutions, but that I have no leisure at present for computations." Newton to Collins, 18th Feb. 1669-70 (see *Macclesfield Correspondence*, vol. ii.). Quoted in the *Journal of the Institute of Actuaries*, vol. viii., p. 64 [1859] by De Morgan, who adds in a footnote: "This is, I believe, the earliest manifestation now known of the temperament which afterwards put the world in some danger of never knowing Newton's discoveries." The letter was written when Newton's age was twenty-six. De Morgan adds: "from the letter cited it seems that Newton expanded the logarithm of a root of any binomial equation."—[*Per* Mr. G. J. Lidstone.]

724. Taylor's theorem will go down to posterity . . . but what does posterity know of the deceased Taylor? *Nominis umbra* is rather a compliment; for it is not substantial enough to have a name.—Bagehot, *Macaulay's Lit. Studies*, ii. p. 9, edit. 1895.

725. Not for "general readers," those somewhat luxurious but presumably intelligent persons for whom so much scientific knowledge is chopped and spiced at the present day.—Ray Lankester [in Huxley's *Crayfish*], N. 21, p. 352.

726. Il faut être profond dans l'art ou dans la science pour en bien posséder les éléments.—Diderot.

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